forest (broken up as it is into bars of light and shade), by the eyes of their fierce and hungry foes as well.

A careful examination of the varied stripes of the zebra has forced upon my mind the conviction that they have a still deeper meaning and value than has hitherto been noticed and explained.

It is easy to see how the vertical bars may assimilate to the falling shadows in the noonday sun, and the diagonal stripes on the neck and hind-quarters to those cast by the declining day. But it is not so much in the day-time and during its waking hours that the zebra stands in such pressing need of concealment as at night, when it is compelled to rest. *Then*, when surrounded by eager and wakeful foes, it *does* require all the concealment it can get. Now, let us suppose the animal to be lying down, say partly on its side and partly on its belly, as horses very frequently do. What will be the effect of such an attitude upon the different stripes on various parts of the body? In the first place, the animal will thrust out its knees, and fold its fetlocks backwards under its body in such a manner that the horizontal bars on the fore-legs will become vertical.

At the same time it will push out in a backward direction, its haunches, and the hind-feet will be brought forward under or near its body, so that the diagonal stripes on the hind-quarters will be *drawn* so as to become much more vertical, and to correspond with the now vertical bars across the hind-legs, made vertical by the folded position of the limbs. In such an attitude—a perfectly natural and common one—all the stripes of the body will be vertical, or nearly so, especially if the zebra rests its head upon the ground, or its fore-legs, so as to bring the diagonal stripes of its neck into unison with all the rest. Supposing, then, that a coincidence in the general direction of the stripes is produced by such an attitude of the body during rest, is it too much to assume it to be an extension and refinement of those protective devices of Nature, extending to the sleeping zebra *the full amount of all the possible protective value of its stripes just at the very time when it needs it most*, so that in the clear tropical moonlight, when the shadows are only a little less distinct than in the day, it may be able to repose in something like safety and peace?

But, suppose the zebra rests, not always on its belly, as suggested, but now and then on its side, with its limbs outstretched. It is plain that the vertical, diagonal, and horizontal stripes would then be all more horizontal than anything else, *but pointing in different directions*, and would then so assimilate themselves with the crossed and varying directions of the shadows as to have the same practical effect in hiding the sleeping animal from its foes.

Under such a supposition (by no means an impossible one), it seems to me that those beautiful bars of brown and white surround the dormant zebra with a protection and a defence almost as secure as bars of iron or brass, leaving the foes with nothing but their sense of smell to guide them to their prey.

We have only to assume the folding up of the limbs, like the folding up of a two-foot rule, *until the marks on both sides correspond*, and we see at once the unification in the general direction of all the stripes of the body, which I cannot help believing has a very considerable protective value to the zebra.

However, if any enlightened and generous patron of science would kindly present to our College ("Owens," Manchester) a good stuffed specimen of a recumbent zebra in the attitude I have suggested, he would help considerably to settle a nice point in the matter of protective colouring, and give to the cause of scientific education a very welcome and appreciable aid.

December 21, 1891. H. W.

The Migration of the Lemming.

There are two questions which I should like to ask Mr. W. Duppa-Crotch touching his recent letter on the above subject (Nature, December 31, p. 199); and for this purpose I had better begin by quoting a paragraph from my own discussion of the same subject, written close upon ten years ago:—

"Looking to Mr. Collett's large experience on the subject, as well as to the intrinsically probable nature of his views, I think we may most safely lend countenance to the latter. The most important point of difference between Mr. Crotch and Mr. Collett has reference to a question of fact. For while Mr. Crotch states that the migrations are made westwards without reference to the declivities of the country, Mr. Collett is emphatic in saying that '*the wanderings take place in the direction of the valleys*, and therefore can branch out from the plateaux in any direction.' If this is so, there is an end of Mr. Crotch's theory, and the only difficulty left to explain would be why, when the lemmings reach the sea, they still continue on their onward course to perish in their multitudes by drowning. The answer to this, however, is not far to seek. For their ordinary habits are such that when in their wanderings they come upon a stream or lake, they swim across it; and therefore when they come upon the coast line it is not surprising that they should behave in a similar manner, and, mistaking the sea for a large lake, swim persistently away from land with the view to reaching the opposite shore, till they succumb to fatigue and the waves. Therefore, pending further observations on the question of fact above alluded to, I cannot feel that the migration of the lemming furnishes any difficulty to the theory of evolution over and above that which is furnished by the larger and more important case of migration in general, to the consideration of which I shall now proceed" ("Mental Evolution in Animals," pp. 284-85).

Mr. Duppa-Crotch's theory thus alluded to—which constituted the most striking feature of his "rather lengthy paper before the Linnean Society," and which, he then wrote, "led me to spend two years in the Canaries and adjacent islands"—is, briefly, as follows:—

"I allude to the island or continent of Atlantis. . . . It is evident that land did exist in the North Atlantic Ocean at no very distant date. . . . Is it not then conceivable, and even probable, that, when a great part of Europe was submerged, and dry land connected Norway and Greenland, the lemmings acquired the habit of migrating westwards, for the same reasons which govern more familiar migrations? . . . It appears to me quite as likely that the impetus of migration towards this continent should be retained, as that a dog should turn round before lying down on a rug, merely because his ancestors found it necessary thus to hollow out a couch in the long grass" (Linn. Soc. Journ., vol. xiii. p. 30).

And, in a subsequent paper (*ibid.*, p. 157 *et seq.*), he combats the statement of Mr. Collett, "that these migrations follow the natural declivities of the country." Now, however, it appears that Mr. Collett turns out to be right as to the fundamental fact of the migrations not being westerly more than towards any other point of the compass; for Mr. Duppa-Crotch, in his letter to you, acknowledges that, in regard to this point, which he previously maintained against Mr. Collett, he "was betrayed into an error by trusting to common report and insufficient personal experience." Nevertheless, he still maintains that the lemmings in their migrations "do not follow the water-shed."

The questions, therefore, which I have to put are: (1) What are the grounds on which Mr. Duppa-Crotch continues to differ from Mr. Collett touching this minor point?; and (2) Does he still maintain his theory with regard to "the island or continent of Atlantis," since he has found himself in error upon the major point? George J. Romanes.

Christ Church, Oxford, January 6.

Destruction of Immature Sea Fish.

It might be supposed that the "importance of the subject" would have induced Mr. Walker, at all events, to examine Dr. Fulton's observations at first hand, before criticizing them (Nature, December 24, 1891, p. 176), instead of confining himself to reading a review.

It may be pointed out that Dr. Fulton's computation of the number of young fish captured is intended to apply only to the Solway, as indeed may be gathered from your review, and being, not a matter of hearsay, as implied by Mr. Walker, but founded on an average of fifteen hauls extending over nine months of the year, is likely to be pretty near the mark.

In examining Mr. Walker's own computation, we find that he reckons six days' fishing to the week, instead of four, which is Dr. Fulton's estimate, based on local inquiry; and we may say that, if Mr. Walker has succeeded in utilizing every working day during any one year of his trawling career, he must have been singularly fortunate in his weather, or must have confined himself to very sheltered waters. I think it will be conceded that a calculation derived from actually counting the catch is more trustworthy than one derived from an observation (or was it only an estimate?) of weight. If, however, "10 cwt. of young flukes, . . . not one the size of half-a-crown," is really only the

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sixtieth part of a day's destruction in the Formby Channel, it is a wonder that there are any left.

A point emphasized in the review, but seemingly missed by Mr. Walker, is that the young fish are always promptly returned to the sea by the Solway shrimpers, and the fact that the industry flourishes in spite of the delay so caused shows that the destruction which ensues from the practices described by Mr. Walker is quite unnecessary. Dr. Fulton has experimentally proved that the proportion of young flat-fish of a certain size (say above an inch) that would not survive if returned to the sea is small, so that it is evident that Mr. Ascroft's "axiom that 90 per cent. of fish that come on board a boat is destroyed" holds good from no fault of the trawl itself, but simply from a discreditable carelessness on the part of the man.

Mr. Walker's experiences at the mouth of the Dee show that the shrimps and the young soles (species?) have different habitats in that river, so that his suggestions as to the limitation of shrimp-trawling seem rather superfluous, since it may be supposed that the trawler would fish where he knew he could get shrimps, and not go out of his way to catch what he did not want. I have noticed myself on the west coast of Ireland that the minute post-larval flat-fish, smaller than those dealt with by Fulton, and which are undoubtedly killed by the meshes of the shrimp-trawl, were never taken on ground frequented by shrimps, where, indeed, as one may judge from the relations of the two forms in captivity, the weaker would have a poor chance of surviving.

Everyone will agree with Mr. Walker that it is most necessary to ascertain the habitat of the young fish at different times of the year, and to this end the energies of the Marine Biological Association in England, the Fishery Board in Scotland, and the Royal Dublin Society in Ireland, have been for some time directed; and the assistance that might be rendered by a series of observations by one possessing the experience and opportunities of Mr. Walker would be incalculable. Until, however, our knowledge on the subject is much more complete, I question the advantage of strewing boulders about the bottom of the sea. Even if they remained to accomplish their purpose of interfering with trawling, there is the danger that they would form an attractive shelter, not to the young flat-fish that stand in no need of it, but to some of their natural enemies.

Dublin, December 27, 1891. ERNEST W. L. HOLT.

A New Precessional Globe.

To facilitate the understanding of the effects of precession, I have made a new arrangement of the celestial globe. A globe mounted in the new way can give a representation of the starry heavens for every place on the earth, and for any date, both past and future.

The globe is fastened in a ring, so that it can be turned round an axis that goes through the poles of the ecliptic, but can also be fixed in any position by a pair of screws. The amount of turning is to be measured by a divided circle.

The ring above mentioned—which we will call ring I.—is movable in another ring (ring II.), round an axis, which forms a right angle with the axis formerly mentioned. The inclination between ring I. and ring II. can be measured by an index; it must equal the obliquity of the ecliptic.

Ring II. is fastened finally in a third and extreme ring (ring III.), so that it can be turned round an axis which forms an angle of 90° with the axis of ring II. Ring III. is mounted on a stand with a horizon-circle, so that its axis can be inclined at pleasure to the plane of the horizon-circle. The inclination may be read on a scale engraved on ring III.

To adjust the apparatus to show the firmament at any appointed place and time, one must place ring III. so that its inclination towards the horizon-circle equals the latitude of the place. Then ring II. must be turned so that its plane coincides with the plane of ring III. The angle between I. and II. must be equal to the obliquity of the ecliptic at the appointed time. Finally, the globe must be turned round the axis which goes through the poles of the ecliptic, till the point of the heaven, which is the celestial pole for the time appointed, comes under the axis round which ring II. turns in ring III. If the globe is then fastened in ring I., and ring I. in ring II., with screws, by turning the globe in ring III. one can see at a glance which stars are setting and rising, and which are always above the horizon.

By making Vega, for example, the celestial pole (14,000 A.D.), one can see immediately that for the latitude of London at that remote period, the Cross would be seen at the southern horizon, and that Sirius then did not rise at all. K. HAAS.

Vienna.

Simple Proof of Euclid II. 9 and 10.

IN NATURE of December 24 (p. 189) a simple proof of Euclid II. 9 and 10 is given, which it is stated is believed to be new. It may therefore be of interest to your readers to know that these proofs are given in an edition of Euclid which we have now in the press. As the author, Mr. Brent, is resident at Dunedin, New Zealand, we are unable to state whether he lays claim or not to any originality in respect to them: in any case, as he has been engaged in mathematical teaching for many years, these and similar proofs of other propositions in Euclid II. have clearly been more widely employed than has been supposed. PERCIVAL AND CO

34 King Street, Covent Garden, London, January 4.

THE ALLEGED DISCOVERY OF A BACILLUS IN INFLUENZA.

FROM the behaviour of influenza as an epidemic, it seems not unreasonable to suppose that it may have as its cause a living and multiplying organism; and when influenza reappeared, after an interval of many years, in the latter part of 1889, and more especially since its communicability from person to person, formerly disputed, has come to be generally admitted, the public mind, medical and lay, has been in expectation of the announcement that a specific microbe had been discovered as the cause of the disease.

Even in diseases, however, of which the characters point most strongly to a parasitic microbe as their cause, the discovery of such an organism is by no means an easy matter. Thus, no micro-organism has as yet been identified as the cause of small-pox, although this disease is always more or less with us; breeds true; has distinct characters, and a definite localization on the skin; and propagates by a contagion which retains its activity for very long periods—circumstances which point to a specific organism as its cause, and might be thought to facilitate its discovery.

From *a priori* considerations we must suppose the properties of the hypothetical influenza microbe to be as follows. The diffusibility of the poison through the air shows that it must be very minute and readily suspended. For the same reason it must belong to the class of aërobic organisms, *i.e.* those for whose existence oxygen is necessary, or at any rate not hurtful. It must multiply with extreme rapidity. It must be capable of multiplying in the bodies, or secretions, of human beings; and probably also in some medium or media outside the human body—perhaps on damp ground-surfaces, or in confined air laden with dust and organic matter. One can hardly suppose it capable of multiplying in pure air, as it would lack pabulum; perhaps, as Dr. Symes Thompson suggests, particles of organic dust floating in the air may serve as rafts for it to live on. As, however, influenza prevails under the most opposite conditions of season, climate, and weather, our supposed microbe, if it can live in the air, must be able to flourish under a great range of temperatures and degrees of humidity. I am not aware of any instances of long retention of contagion, such as would lead us to postulate the possession by our microbe of resting spores. From these considerations we might have expected that it would be more likely to turn out to be a micrococcus than a bacillus.

From what is known of the pathology of some other diseases of microbic origin, as tetanus and diphtheria, it seems possible that the immediate cause of the symptoms of influenza may be the presence in the blood and tissues, not of the microbe itself, but of the poison

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